

Astoria, Portland, and Ft. Vancouver. In September he was among the mountains of the upper Columbia River, but by October he had returned to the Sierra Nevada of California. It does not appear when he finally left California, but by the middle of December 1870 he was again in Panama. The first two months of the new year were devoted to revisiting Columbia, after which ROEZL returned to Europe.

It appears from this account that the only opportunity which ROEZL had of procuring seeds of *Washingtonia* was during his visit to San Diego, in December 1869. The notes, however, contain no reference to this palm. But a visit to any of its desert habitats would certainly have been an experience too notable to have failed of record. Nor is it probable that his visit to San Diego, so short and so diligently occupied in collecting, could have afforded time for the difficult journey to the desert. The vague and confused habitat assigned to the palm is itself a sufficient evidence that the collector, from whom the information must have come, could never have visited a native grove. It is safe to conclude that the seed he sent to Europe came from some of the older cultivated trees at San Diego, and that his pardonable ignorance of local geography prevented him from correctly understanding what was told him of the location of the indigenous groves.—S. B. PARISH, *San Bernardino, Cal.*

LONGEVITY OF SEEDS

In the *BOTANICAL GAZETTE* for January 1909, p. 69, CROCKER, in referring to my paper on this subject, concludes with the remark: "I believe I am doing the author no injustice when I say that it is impossible to tell from his paper in how far it is a contribution and in how far a compilation." May I say that the utmost care was taken to quote the authority for every record or fact that was not original, and that I am unable to find a single case in which this was not done. If any such omission occurs it is a purely accidental one, and I am prepared to offer both a public and a private apology to any author whose name is omitted as the authority for a record for which he is responsible. Naturally, however, if on repeating a test or experiment a more or less divergent result is obtained, the original authority can hardly be given for the changed statement of fact, which in many cases directly negatives the original record. The latter, however, is given in all cases with the author's name appended, so that it is difficult to see any foundation for CROCKER'S criticism.—ALFRED J. EWART, *University of Melbourne.*

REJOINDER

In spite of EWART'S very energetic objection to my criticism of his paper, I maintain that the criticism is entirely justifiable. To escape personal bias I have asked several persons to read passages of EWART'S article and to say who contributed the data. In every case they decided that they were EWART'S, though this is not the case.

As an example, I cite the first part of the paragraph beginning at the foot of p. 197, dealing with *Plantago major*, *P. Rugelii*, *Thlaspi arvense*, and *Avena fatua*. The data are all given in my article,⁴ but one would not know from EWART'S statement that this was their source. Again, in the paragraph beginning near the top of p. 192, EWART gives the arguments against FISCHER'S conception of the cause of delayed germination in the seeds of water plants as if based on his own work. All these arguments are given in another paper of mine,⁵ issued five months before EWART'S. In the same paragraph, he says: "Since the above was written, CROCKER (BOT. GAZETTE 1907, 374) has shown, etc." One would have expected a writer who is so careful to give credit where it belongs to recast this paragraph after he discovered my article, so as to indicate proper priority. I mention these as two instances out of several that furnished the basis of my criticism.

EWART speaks again of his results contradicting mine in a number of cases. I must therefore point out again that the matters in dispute are minor details and not cardinal principles in the physiology of delayed germination; and I should be glad to have anyone compare his paper and my criticism to judge in how far there is evidence that his results disprove mine. I have repeated the experiments upon the disputed points, and have had various competent students do so independently. The results in every case agree with my previous conclusions, as my criticism points out.

I am sorry that what I considered and still consider a fair criticism has led to undue publicity. I am sure, however, that neither a public nor a private apology is necessary from EWART, as the case will rest upon its merits.—WILLIAM CROCKER, *The University of Chicago*.

⁴ CROCKER, W., Rôle of seed coats in delayed germination. BOT. GAZETTE 42:282-284. 1906.

⁵ ———, Germination of seeds of water plants. BOT. GAZETTE 44:375-380. 1907.